1847MNRAS...7..290H

		Δ α cos δ	Δ δ
		″	″
26	Dawes	+ 10·8	− 0·3
	South Villa ...	+ 0·9	+ 16·1
27	Dawes	+ 7·8	+ 9·8
28	Dawes	− 12·2	− 14·3
29	South Villa ...	− 6·8	+ 1·3
30	South Villa ...	− 1·6	+ 1·4

"In the comparison with Mr. Dawes' observations and my own, aberration and parallax are taken into account; in all other observations parallax is neglected, but the aberration is allowed for."

By M. d'Arrest.

Perihelion Passage, 1847, June 6·00465, Berlin M.T.

	°	′	″	
Longitude of Perihelion	141	0	2·1	Apparent Equinox.
——— Node	173	54	37·8	
Inclination	79	36	9·7	
Log. q	0·325492			Motion retrograde.

M. d'Arrest has computed his elements from the Hamburg observations of May 18, and the Berlin observations of May 21 and 24. The Berlin observations are,

		Berlin M.T.			R.A.			Dec.		
		h	m	s	°	′	″	°	′	″
1847.	May 21	11	51	5·6	150	27	4·7	+40	5	4·6
	24	12	16	15·3	150	27	58·0	+40	46	5·6

By M. Quirling (assistant to M. Rümker).

Perihelion Passage, 1847, June 18·52595, Berlin M.T.

	°	′	″	
☊	173	26	59	
☊ − π	38	37	5	
Inclination	99	20	23	
Log. q	0·320584			Motion retrograde.

From the Padua observations of May 13, and the Hamburg observations of May 18 and 22.

Sweeping Ephemeris for the expected Comet of 1264 *and* 1556, *on three hypotheses of the time of Perihelion Passage.* From Mr. Hind's Tables in the *Monthly Notice* for April 1847.

		P. P. Aug. 14.				P. P. Sept. 13.				P. P. Oct. 13.			
		h	m	°	′	h	m	°	′	h	m	°	′
1847. July	15	10	15	17	12	9	59	10	12	9	44	+6	58
	25	10	42	18	44	10	21	10	21	10	1	6	52
Aug.	4	11	10	20	45	10	42	10	16	10	21	6	34
	14	11	32	23	27	11	16	10	8	10	44	6	6
	24	11	34	+26	15	11	52	+9	32	11	10	+5	27

On the Opinion of Copernicus with respect to the Light of the Planets. By Professor De Morgan.

"The common story is, that Copernicus, on being opposed by the argument that *Mercury* and *Venus* did not shew phases, answered that the phases would be discovered some day. The first place

1847MNRAS...7..290H

in which I find this story is in Keill's *Lectures*. It is also given by Dr. Smith, in his well-known Treatise on Optics, by Bailli, and by others. But I cannot find it mentioned either by Melchior Adam or Gassendil, in their biographies of Copernicus; nor by Rheticus, in his celebrated *Narratio*, descriptive of the system of Copernicus; nor by Kepler, nor by Riccioli, in their collections of arguments for and against the heliocentric theory; nor by Galileo, when announcing and commenting on the discovery of the phases; and, what is most to the purpose, Müler, in his excellent edition of the great work of Copernicus, when referring to the discovery of the phases of *Venus*, as made since, and unknown to, Copernicus, does not say a word on any prediction or opinion of the latter.

"This story may then be rejected, as the gossip of a time posterior to Copernicus. If we try to examine what the opinion of Copernicus on this matter really was, a point of some little curiosity arises. It depends on one word, whether he did or did not assert his belief in one or other of these two opinions,—that the planets shine by their own light, or that they are saturated by the solar light, which, as it were, soaks through them. I support the affirmative: that is to say, I hold it sufficiently certain that Copernicus did express himself to the effect that one or the other of these suppositions was the truth.

"If we take the first edition of the work *De Revolutionibus*, which was printed from the manuscript furnished by Copernicus himself, there is little doubt about the matter. There are but two passages which bear or can bear upon the question. The first is in the *ad lectorem*, in which the writer (Osiander, though even Delambre make him Copernicus) asks whether any one acquainted with Geometry or Optics can receive the Ptolemaic epicycle then used to explain the motion in longitude of *Venus?* But the meaning of the allusion to optics is explained in the next sentence, by a reference (and by no means a fortunate one) to the changes of apparent diameter of *Venus* derived from that epicycle; changes which, as they made the perigean diameter more than four times as great as the apogean, were assured to be falsified by common experience. The second passage is the one on which this discussion must turn. In book i. chap. x., after noting that some had theretofore believed *Mercury* and *Venus* to come between the earth and sun, he mentions the difficulty arising from the absence of the remarkable phase, which we now call the transit over the sun's disk. He describes the opinion just mentioned favourably, referring, not to his own view, but to that of those others who had held it. This is not an uncommon idiom: persons advocating an unpopular opinion are very apt to describe the maintainers of it in the third person, though themselves be of the number. But when he comes to describe what he takes to be the necessary consequence of the opinion, he lapses into the first person as follows:—*Non ergo fatemur in stellis opacitatem esse aliquam lunari similem, sed vel proprio lumine, vel solari totis imbutas corporibus fulgere, et idcirco solem non impediri* . . . . .

1847MNRAS...7..290H

"These are the words of the first edition (Nuremberg, 1543). That Copernicus could have answered any objection, either by word or writing, is impossible, since he drew his last breath within a few hours of the time when, not able to open it from weakness, he saw the first printed copy. The second edition (Basle, 1566), is usually said to have been edited by Rheticus. The reason of this is that the name of Rheticus appears in the title-page. But this appearance only arises from the *Narratio*, &c. of Rheticus being added to the edition; and it is only the description of this addition which brings Rheticus into the title-page. There is no mark whatever of his having been the editor; and as the work was printed at Basle, where I cannot find that Rheticus ever sojourned, and as the latter was deeply engaged at the time in his enormous trigonometrical calculation, some proof of his editorship must be given before it is admitted. As the point is of importance, I will notice, that unless Rheticus had made some stay at Basle, it is very unlikely he should have edited a work printed there. He did not edite the first edition, only because it was found convenient to print it at Nuremberg instead of at Wittemberg; and it was accordingly entrusted to Osiander. Now, if ever there were a connexion between two men, and between one of them and the book of the other, which made it desirable and even necessary that the first should edite the second, it was the case of Rheticus and the first edition of the *De Revolutionibus*, &c.; and yet no arrangement could be made by which the sheets printed at Nuremberg could be revised at Wittemberg. It is very unlikely, then, that Rheticus should have edited the second edition, when, as far as we know, a similar impediment existed.

"The third edition, by Müler (Amsterdam, 1617), has no authority as to the text above that of the second.

"Now both the second and third editions change the word *fatemur* into *fatentur*, thus causing Copernicus to throw the opinion in question upon his predecessors, instead of directly making it his own. Not that it would be conclusive, even if the emendation were adopted: for, as I have said, Copernicus is evidently speaking with approbation of the opinions which he describes; and it would be difficult to say why *comperiunt* or *putant* in one sentence should imply approbation, and *fatentur*, in the next, should be at least disavowal, if not disapprobation. If Rheticus, who knew the mind of Copernicus better than any one, had been the editor, I can conceive that stress ought to be laid upon the change of the first into the third person as an emendation; that is, I should be somewhat staggered by Rheticus having thought it necessary to make such an alteration.

"But, Rheticus not being in the question, as I think, for the reasons given above, the next best authority on an opinion of Copernicus is Galileo. Now the latter, in speaking of the phases of *Venus*, expressly attributes to Copernicus the maintenance of one of the two alternatives,—that the planet is either self-luminous or perforated by the solar rays. Of these alternatives, he says, in his

1847MNRAS...7..290H

letter to Velser (*Works*, vol. ii. pp. 88, 89), *Al Copernico medesimo convien amettere come possibile, anzi pur come necessaria, una delle dette posizioni.* And that such was the opinion of Copernicus is also assumed by the writer of the note on the *Sydereus Nuncius* in the volume just mentioned, and by others, even down to our own time; as by Mr. Drinkwater Bethune, in his life of Galileo. In fact, with the exception of the unsupported story mentioned at the beginning of this paper, there is nowhere, that I can find, any thing against my conclusion. And it is to be remembered, that Copernicus nowhere shews any of that acumen in matters of physics, apart from mathematics, which has often enabled the cultivators of the former to make steps more than proportionate to their knowledge of the latter. Ptolemy, the great promoter of the old theory, and Copernicus, its destroyer, were both mathematicians in a peculiar sense; Ptolemy being far the more sagacious in questions of pure experiment. Their grounds of confidence are mathematical; and Copernicus, in particular, dares to face his own physics (for there is no reason to suppose he was beyond his age in mechanical philosophy) with reasons drawn entirely from probabilities afforded by mathematics.

"There is much reason to regret the practice of associating with the names of those who have led the way in great discovery the glory which is due to their followers. The disadvantage is twofold. In the first place, it introduces into the history of science an index error of from one to two centuries; secondly, those who come to inquire are disappointed to find that they must lower their opinion of great men, and are perhaps led to do it to a greater extent than justice requires. Our usual popular treatises speak of Copernicus as if, besides himself, he had in him no inconsiderable fraction of Kepler, Galileo, Newton, and Halley. What is a person to think who comes from those histories to actual investigation, when he finds in Copernicus himself the immovable *centrum mundi* (only reading sun for earth) of the Ptolemaists, their epicycles, and a suspicion, at least, of the solid orbs?"

Notice of a Letter from Mr. Maclear, dated from the Kamies Berg, Cape of Good Hope.

Mr. Maclear started for the north end of his arc on the 28th of September last. After scouring the Bushman flat to select stations, Mr. Maclear occupied the northern station of 1843; and Messrs. Campion and Montagu took the side stations, on the plan of working simultaneously at the two angles of a triangle, a considerable saving of time and trouble when heliometers are used. The triangulation has been carried on till the date of his letter, April 27, when the party was preparing to march to the northern stations to make the sector observations. "The flat is now comparatively cool; in December, it was hot indeed, the perspiration streaming down, and the sensation being that of being enveloped in a perpetual poultice." The change of climate from the plain to a

1847MNRAS...7..294.

mountain above 5000 feet high, brought on a rheumatic affection of the heart and diaphragm, from which Mr. Maclear is as yet, unfortunately, not free.

The Fuller theodolite, which was lent by the Society to the Lords of the Admiralty for this survey, seems not to have been adapted to the work; but the mode of using it, though very laborious, will remedy any faults in the construction. Each angle taken with the Fuller theodolite is measured at least eighty times, on forty successive arcs, each arc twice. The other theodolite is an eight-inch repeater, by Reichenbach. Ten repetitions make one measure, ten measures are considered a set. There are always two sets taken, and sometimes three or four.

The suffering and anxiety have been excessive. In a country so desolate, every thing, even water, has to be brought from a distance. "Frost and snow," Mr. Maclear remarks, "may be disregarded,—to battle with these we have fire and clothing; but there is no escape or remedy against heat, no substitute for water. If I had been aware of what I should have to undergo in the last six months, I think I should have shrunk from it."

The laborious part of this great undertaking* seems, however, to be now coming to a close; and Mr. Maclear looks with great delight to the comfort of regular observation.

On an Improvement in Tables of Proportional Logarithms.
By Mr. Drach.

Mr. Drach's proposal is founded on Lieut. Raper's remarks (See Raper's *Navigation*, second edition, end of Explanatory Tables), viz. that when the difference of lunar distances exceeds 1° 13′, the nearest second is not always found from the proportional logarithms now in use.

This remark is perfectly correct; but when all the circumstances involved in the problem and its application are taken into account, it may be doubted whether any real advantage would be found, in *practical navigation* at least, from the improvement proposed.

On the Formation and Application of Fine Metallic Wires to Optical Instruments. By Mr. Ulrich.

Dr. Wollaston, in the *Philosophical Transactions* for 1813, proposed a method of forming wires of gold or platinum of any degree of tenuity. The discovery does not appear to have been much used, owing, as Mr. Ulrich supposes, to the difficulty of application.

Mr. Ulrich forms the fine wire by inserting a gold or platinum wire in the centre of a silver cylinder of much larger dimensions, which is afterwards drawn out by the usual process. When the silver wire has been sufficiently extended, Mr. Ulrich cuts it into

---

* See the last Annual Report for an account of Mr. Maclear's arc.